I am so strongly tempted unreservedly to accept the "protective" theory, that I perhaps lay too great stress on these negative instances. As a matter of fact, I suppose that the experience of a single individual is rarely large enough to justify any induction being made from it. I myself, for instance, have never come across the extreme variations of the cuckoo's egg, such as Seebohm figures. E. B. TITCHENER.

3 Museum Terrace, Oxford, December 3.

Is the Bulk of Ocean Water a Fixed Quantity?

MR. MELLARD READE'S criticism is perfectly sound. If the bulk of the ocean water on the surface of the globe has always been the same, the oceans could not at any time have been shallower than at present without a decrease in the area of the land. Consequently, the supposition that in early geological times the area of the land was larger, and the depth of the oceans less, demands the further inference that the bulk of the ocean water was less then than it is now.

When writing on the physics of the sub-oceanic crust, I saw that this was a necessary consequence of the theory, but I was not then quite prepared to discuss it. I have since had some correspondence with Prof. A. H. Green and Mr. O. Fisher on the subject, and will briefly indicate the possibilities that have occurred to us.

The first suggestion made was that, if the solar radiation was greater in Palæozoic times, there would be greater evaporation, and as the temperature of the air would also be higher, the atmosphere could hold more aqueous vapour than it does now, so that we might suppose a part of the water which is now in the ocean to have been then permanently suspended above it. Mr. Fisher, in writing to me, admits this possibility, and even thinks it might be feasible to estimately roughly the amount of water so suspended if the mean temperature of the ocean at any period was known. But he says:—"I do not think you could get much diminution of the oceans in this way, for, suppose the present atmosphere to consist of nothing but aqueous vapour, then it would represent a layer of water about 30 feet thick evaporated from the earth's surface. Now, it seems hardly probable that at a former time there should have been an amount of aqueous vapour in the atmosphere so great that the mass of such additional vapour should equal that of all the oxygen and nitrogen and vapour now in the atmosphere; and even if there was this amount, it would take off only about 30 feet of water from the surface of the globe," or about 37 feet from the present surface of the oceans.

If, therefore, the bulk of the water on and above the surface of the earth has remained the same since the time when the crust was first formed, it seems difficult to find any means of sensibly diminishing the amount of water in the oceans. But need we make this preliminary assumption, and is it not really possible that there has been an increase in the bulk of surface-water, and not a decrease by absorption, as some theorists would have us think? May we not suppose, in fact, that water-substance has always existed in the interior of the earth, and may it not, by its constant and gradual escape, have always been adding to the bulk of the surface-waters?

This idea had occurred to Mr. Fisher so long ago as 1873, and the following passage occurs a paper then published (Trans. Camb. Phil. Soc., vol. xii., Part 2, p. 431): "If such was the condition of the interior in the early stages of the cosmogony, a large portion of the oceans now above the crust may once have been beneath it"; and in the new edition of his "Physics of the Earth's Crust" he further discusses the manner in which this water-substance may be diffused through the magma of the liquid substratum beneath the crust.

As a matter of fact, it is well known that almost all volcanoes, when in eruption, emit large quantities of steam, and the presence of this steam has always been connected with the causes of volcanic activity. There are only two ways of accounting for the presence of this steam: (1) that water from the sea or from the rainfall gains access to the deep-seated foci of volcanic action; (2) that the water-substance is a primary constituent of the liquid magma below, and that when this material is forced up to the surface, the pressure which kept the water in solution or combination is removed, and it is blown off as steam.

As regards the first possibility, there are great difficulties in the way of supposing that surface-water can find its way to any region where the heat is sufficient to keep rock constantly in a liquid condition. It does seem possible that the access of water to the interior parts of a volcano *already established* may sometimes cause an eruption, and, under certain circumstances, an eruption of great violence; but the descent of water through the earth's crust to depths of 20 or 30 miles so as to be the initial cause of the establishment of volcanoes is not so easy to understand. The pressure of the superincumbent rocks at a depth of 2 or 3 miles must be so great that all cracks and interstitial spaces would be reduced to a minimum, and at the depth of 5 miles one would suppose that none such could exist. Several facts are known to geologists which show that all cracks diminish rapidly downwards. One such fact is that in many deep mines the throw of a *fault* diminishes with the depth to which it is followed. Another is the existence of such warm springs as those of Bath, the explanation of which is supposed to be that water percolating downward (say from the Mendips) reaches a depth at which there is less resistance to its travelling laterally than to its further descent, and that ultimately reaching a crack or fault, it is forced up this path of least resistance by the hydrostatic pressure of the descending stream.

It is true that a residuum of the water might continue its downward journey, being, as it were, slowly sucked downward as far as the minutest interstitial spaces extended; but what would happen when it reached the lower layers of the crust? Could it possibly reach and be absorbed by or dissolved in the semi-fused rock which must there exist? Captain C. E. Dutton has well expressed this difficulty. Referring to the high temperature which must exist at a depth of 5 or 6 miles, he says:—"At such a temperature the siliceous materials of which the rocks are composed are no longer hard and brittle as when they are cold, but viscous and plastic. . . . Now a crack or fissure might reach very far down into hard, cold, brittle rocks, but into soft semi-fused plastic rocks, never. Under a pressure of several miles of superincumbent strata, a crack, or even the minutest vesicle, would be tightly closed up as if its walls were wax or butter. A more perfect packing against ingress of water could not be conceived." [1]

Even capillary action could not come into play under such conditions as these.

Let us next consider the alternative theory suggested by Mr. Fisher. He claims that geologists furnish him with a certain amount of positive evidence for the idea that water is an essential constituent of the liquid magma from which the igneous rocks have been derived. Passing over the proofs of the existence of water in the crystals of volcanic rocks and in the materials of deep-seated dykes, let us come at once to granite, a rock which can only have been formed at great depths and under great pressures, and which often forms large tracts that are supposed to have been subterranean lakes or cisterns of liquid matter in direct communication with still deeper reservoirs. Now, all granites contain crystals of quartz, and these crystals include numerous minute cavities which contain water and other liquids; and the quartz of some granites is so full of water-vesicles that Mr. Clifton Ward has said: "A thousand millions might easily be contained within a cubic inch of quartz, and sometimes the contained water must make up at least 5 per cent. of the whole volume of the containing quartz." This amount only represents the water that has been, as it were, accidentally shut up in the granite, for some was doubtless given off in the form of steam which made its way through the surrounding rocks.

It is therefore generally conceded that granite has consolidated from a state of igneo-aqueous fusion, and that the liquid magma from which all granitic intrusions have proceeded contains water-substance. It is therefore only a step further to assume that this water-substance is an essential constituent of the liquid substratum, and to suppose that it has been there since the consolidation of the earth. That there is no inherent improbability in this supposition, and that it is not inconsistent with chemical views of cosmogony, Mr. Fisher has shown at the end of his chapter on the "Liquid Substratum."

I am only now concerned with it as an explanation of the secular increase in the bulk of the ocean waters which is demanded by my theory of the evolution of continents and oceans. We can prove from the geological records that volcanic action has always been in operation from the very earliest times in the world's history, and if it is true that such a reservoir of water-substance has always existed in the earth's interior, the continual volcanic eruptions must have constantly added water to the oceans on the earth's surface. Hence, as I stated in my

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[1] "Volcanoes," by C. E. Dutton, in *Ordnance Notes*, No. 343, Washington, 1889.

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first letter, we are at liberty to imagine a time when there was much more land than there is at present, and when all the oceans were comparatively shallow. A. J. JUKES-BROWNE.

Galls.

BEFORE rushing into arguments on this subject, it appears to me that more good might be done by entering into investigations of the physiological and morphological problems involved.

A gall-fly of a particular species inserts an egg in a certain position on a certain plant (oak, for instance). Another gall-fly of a different species inserts its egg almost in the same position on the same plant. But the results are totally dissimilar. An abnormal growth is set up, from irritation, in either case; but the nature of this growth is quite different. The initial irritation is set up by the presence of the egg, and in most gall-insects the egg *grows*—that is to say, it increases vastly in size before the larva is hatched. The irritation is continued by the larva, and the gall is produced, varying in form in accordance with the species of gall-fly that deposited the egg. But I want to know in what consists the difference in the active irritation that causes so great a divergence in the results? I am not aware that this has ever been answered. But I am quite sure it could be answered on purely physiological grounds if carefully studied. The answer would not in the least detract from the importance of the point as regards natural selection; but it might very materially modify speculative theories based on results only, without a precise knowledge of the agencies that produced those results. R. McLACHLAN.

Lewisham, November 29.

ALTHOUGH I see no need of a better explanation than Prof. Romanes's (NATURE, November 28, p. 80) of the difficulty which galls seem at first sight to present for natural selection, yet I beg leave to say some words of further elucidation.

When it was said by Darwin ("Origin of Species," chap. vi.): "If it could be proved that any part of the structure of any one species had been formed for the exclusive good of another species, it would annihilate my theory, for such could not have been produced through natural selection," he evidently meant only species living without organic connection with each other, viz. his own example of the rattlesnake. The argument does by no means apply to organisms living in a relation of *symbiosis*, as is the case with gall-bearing plants and the larvæ inhabiting the galls.[1] Such associations form, as it were, one compound organism. Natural selection evidently may act in favour of each symbiont separately, provided only that the effect will not damage the other symbiont in such a degree as seriously to impair its existence. Some "disinterested" expenditure of energy and of organic substance is not excluded by natural selection, but may be promoted, if of advantage to the other partner. Thus the production of galls will scarcely do any serious injury to an oak, and even if such were sometimes the case, there would be no comparison to the damage worked, for instance, by Trichinæ, on the organism of man and animals, which hosts, nevertheless, in consequence of the stimulus caused by the parasite, afford the substance for capsules protecting the worms, just as plants produce manifold structures beneficial to the gall-insects. If Trichinæ would attack a species of mammals as frequently as, for instance, leaf-cutting ants attack some tropical plants, then those hosts would be forced either to develop, by survival of the fittest, some protection against their invasion, or they would succumb to the enemy and die out.

Analogous examples might be multiplied of both plants and animals, and it is especially to be remembered, as alluded to by Prof. Romanes, that the chemical activities of parasites, including the elaboration of ferments affecting the saps and tissues of the host, are as much under the guidance of natural selection as are their morphological variations. D. WETTERHAN.

Freiburg, Badenia, November 30.

WITH all due deference to your able correspondents Dr. St. George Mivart and Prof. G. J. Romanes, I cannot for the life of me understand how the theory of natural selection can be seriously assailed by investigations into the formation of galls by insects. Gall-formation has always appeared to me to be a pathological, that is a *perverted physiological* process, and to be due to the action of some animal irritant upon normal vegetable tissues during their period of active growth. These formations are therefore, to my mind, fairly on a par with the globular nests produced by the larvæ of the Œstrus, or bot-fly, in the hides of oxen; or to the inflammatory foci in the tissues of the kidneys, due to the translation of Bacilli, in the case of ulcerative endocarditis. Other examples bearing on the subject will doubtless occur to your readers. In all such instances we have certain changes in the cellular or protoplasmic tissue-elements of the host, brought about by the growth and development of a foreigner in their midst; and natural selection, in so far as it operates in such cases, seems to have sided mostly with the stranger, and to be to his advantage alone. That the host under these circumstances performs actions "which, if not self-sacrificing," are at least "disinterested," must be admitted; but it is the self-sacrifice of coercion and disinterestedness under compulsion. W. AINSLIE HOLLIS.

Brighton, December 1.

Luminous Night Clouds.

THE many inquiries and appeals regarding observations of luminous night clouds which have recently appeared in the columns of NATURE, and the growing importance of the subject, will justify me, perhaps, in sending to you, for publication in that journal, the following item, so long after the event it describes took place.

About the middle of November 1887, between eight and nine in the evening, as I was walking homewards from my day's work, I noticed what appeared to me to be the arch of a rainbow very low above the western horizon, and of a snow-white colour. A bank of clouds was rapidly approaching from the west, which, at the time of the first appearance of the arch, covered nearly half the sky, the eastern half being clear. The arch appeared to move eastwards, with and in the midst of the clouds, for it continually rose above the horizon, and, in the course of about half an hour, had approached the zenith.

At this time I called out several people to witness the phenomenon, which certainly presented a most extraordinary appearance. The arch appeared to be uniformly of about 3° or 4° in width, and extended north-north-east and south-south-west across the whole sky. The latter was about wholly overcast with the clouds at this time, except the arch, which presented a glaring brightness, and illuminated the earth with a weird splendour four or five times exceeding that of the brightest moonlight.

While at the zenith, the stars shone through the entire width of the arch with apparently more than ordinary brightness; but as the arch approached towards and receded from that point, the width of the transparency was observed to diminish rapidly with the distance, until at 10° or 15° on either side the stars were invisible through it.

The phenomenon appeared to be a division in the cloud stratum, the opposite walls of which were pretty clearly defined; and there appeared to be absolutely nothing between these opposite cloud walls but the purest air and the white light of the arch. I remember also that the wall or border of cloud on either side of the arch was slowly revolving upon an axis parallel with the arch; just as is often seen in the front bank of clouds of an approaching storm. But I do not remember the direction of the rotation, or whether both borders rotated in the same or in opposite directions.

The arch moved towards the east at about the same pace that it approached from the west, and with apparently the same width and direction of extension. There was no moonlight at the time, and only a gentle breeze was blowing. The weather preceding the phenomenon was fine for several weeks; but a few days afterwards, or on November 19, there was a sudden and extraordinary fall of the temperature, accompanied by some snow and very high wind.

I have thought that possibly this phenomenon might throw some light on the subject of luminous clouds, and that this tolerably accurate description of it may therefore be of interest to the students of that subject. I may add, however, that the luminosity of the arch did not appear to proceed directly from the clouds themselves, but from the clear space between the

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[1] Darwin's thorough acquaintance with these important structures is shown by his elaborate discussion in "Animals and Plants under Domestication," chap. xxiii. (2nd ed. vol. ii. p. 272). It is particularly to be noted that Darwin insists on the accordance of galls, for instance, on roses, with structures arising through bud-variation.

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